Perspective from Practice

Searching for Significance: The Case for Reimagining Management Research

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Anyone trained as a management researcher would presumably agree that the goal of management research is to create new knowledge, but by what standards should such knowledge be judged? Clearly there's an expectation that it should be scientifically robust, but beyond that—what? Given that management is an applied field of social science, we would suggest three additional criteria:

- *Novelty*—The knowledge should be non-incremental, frame-breaking, and more than a turgid and unnecessarily complex restatement of what a smart practitioner would already know or assume.
- *Salience*—The knowledge should speak to some timely and essential problem facing a significant number of practitioners.
- *Usability*—The knowledge should be practically useful, with implications for practice are clearly enumerated.

If one accepts that these are appropriate standards for judging the output of management researchers, an obvious question follows: What could management researchers do to achieve a 10X increase in the impact of their work? If this question suggests we are disappointed by the impact of management research to date—well, we are.

Doubts About Impact

It is immensely difficult to measure the impact of management research on practice, but we can identify a few proxies.

Consider that only 5% of the members of the Academy of Management are practitioners. In the US there are about 20 million individuals with a managerial role, and another 7 million in administrative support roles. Of these 27 million souls, only 900 belong to the Academy of Management. That's 3 out of every 100,000 managers—not exactly a big vote of confidence in the ability of management researchers to produce useful knowledge. Or consider the Strategic Management Society. Despite its goal "to bring together the worlds of reflective practice and thoughtful scholarship," only 160 of Its 2,900 members identify themselves as business executives or consultants.

One of the author's has a wife who's an obstetrician, and she, like most practitioners in her field, reads journals like *Obstetrics and Gynecology*, and *The American Journal of Obstetrics and Gynecology*. These are top journals for researchers and practitioners alike.

In OB/GYN, as in most medical disciplines, there's little demarcation between "academic" journals and "practitioner" journals. Just about everything published in leading journals is meant to be clinically useful. And, as a rule, the articles are paragons of concision and clarity.

The point is, well-constructed studies and peer review should help to produce knowledge that is *more* relevant and accessible, rather than less.

Unfortunately, this doesn't seem to be the case in management science. Select at random an article in a top journal and put yourself in the shoes of an earnest practitioner looking for new and useful insights. In all likelihood, they'll find the writing turgid and the arguments banal. Indeed, it often seems that the more baroque the language, the more feeble the intellectual scaffolding it adorns.

The authors aren't at fault—they're simply playing by the rules of academic publishing (as currently constructed). Nevertheless, one can hardly expect practitioners to hack their way through an impenetrable thicket of words to find the few simple truths within.

Another proxy for impact, or at least the seriousness outsiders attach to our research, is research funding. On this point, let us share some data from the US National Science Foundation[1]. In 2018, US corporations spent $4.7 billion funding university research.

Of this, business and management research captured a minuscule 1%.

The bulk of the funding went to research in life sciences (more than $2 billion) and engineering (more than $1 billion). A wide range of sub-disciplines were better funded than management, including agriculture, astrophysics, civil engineering, geology, ocean studies and psychology.

Just as telling is the mix of funding sources.

In health sciences and engineering, over 50% of the funding came from government—and nearly 80%, overall, from non-university sources. Apparently, researchers in these fields were able to convince funding agencies that their work would create positive economic and social externalities—a public pay-off, in other words.

Management researchers, by contrast, seem to have failed to make this point, since external sources account for less than a third of research funding. The upside, one could cynically argue, is that the paucity of outside funding frees management researchers from the need to worry about the relevance of their work to those outside the academy.

Here's a final barometer of impact—in the form of a thought experiment.

Ask yourself …

What would be the cost to our economies and societies if there were a 10-year moratorium on business school research? How many practitioners would notice? Would gains in organizational performance flatten out? Would there be a hit to the global economy? Or would no one notice?

Our suspicion is that the business world would barely notice. In a long ago conference we remember Henry Mintzberg railing against research that was "significant only in the statistical sense," and in 1993, Don Hambrick, then president of the Academy of Management asked, "What if the Academy actually mattered?" Other scholars have voiced similar concerns about the impact of management research. (Rynes, Bartunek and Daft, 2001; Mohrman and Lawler, 2012; Pfeffer and Fong, 2002). Fact is, scientifically robust truth claims are of little value if the findings are operationally insignificant.

So yes, we are disappointed by the impact of management research. Maybe we shouldn't be. Maybe someone would like to do a comprehensive study on the value of management scholarship and put our minds at ease—that would be welcome.

Thanks to the research of Nick Bloom and others, we know there's a pay-off to "good management" (Bloom and van Reenen, 2007)—as if that were ever in doubt. The question, though, is how essential is academic research to advancing the state of the art? Based on the evidence, could b-school academics build a compelling case for governments and businesses investing $1 billion a year in management research? Could we credibly promise a positive ROI?

Some might argue that impact, as judged by practitioners or funding agencies, doesn't really matter. Why worry about relevance if you can dig up the funds to support your research and publishing? After all, artists don't worry over much about impact and they still eat (at least some of them do); neither do theoretical physicists worry themselves with pedestrian concerns like ROI.

But we're not artists—commenting wryly on the human condition. Nor are we physicists uncovering the hidden secrets of the universe. We're engineers. We're charged with improving the effectiveness of human organizations or, if you will, "management."

In theory, management researchers care about relevance. (Pun intended). Consider, for example, the mission statement of the Academy of Management: "The Academy's mission is to advance the profession of management."

Sounds good, but we choke on the word "profession." Management is not a profession[2]. There's no credentialing, no obligatory continuing education, no standards body, and, perhaps thankfully, no malpractice insurance. Obstetrics, by contrast, is a profession. But interestingly, the mission statement of the College of Obstetrics and Gynecology makes no mention of obstetricians. Instead, it says its goal is to "change the practice of women's health care."

Our mission, as management researchers isn't to advance the "profession" of management—what does that even mean in a company like Haier, the global appliance leader that has pancakeflat organization with virtually no formal management positions? No, the mission is to advance the practice of management—because management, like women's health, is vital to human flourishing.

Why Management Matters

Here's another question:

What have been the most important inventions of the past 150 years? What are the things you can scarcely imagine living without? You might think of electricity, antibiotics, cars, contraception, refrigerators, computers, the Internet, and, most of all, your smartphone. As indispensable as these things are, we'd argue the single most important invention over the past 150 years was large-scale industrial management.

None of those scientific inventions would have been affordable if not for the invention of modern management—which gave us the ability to make things of ever-growing complexity with ever increasing efficiency.

Our definition of management is simple: "The structures, methods and tools we use to mobilize and organize resources to productive ends." In other words, it is quite simply, "the technology of human accomplishment."

It is advances in management that gave us the power to turn science into prosperity—to make automobiles, household appliances, commercial air travel and digital devices affordable for billions of consumers. Consider the gains: Over the past 150 years, the value of the output from an hour of human labor grew 11-fold in the UK, 20-fold in the United States, and more than 60-fold in Japan.

The economist William Baumol (1986) has noted that 90% of the productivity gains since 1870 came from innovation—and innovation in management structures and systems played a central role. Today we take many of these innovations—like capital budgeting, divisionalization, brand management, strategic planning, total quality management and agile development—for granted (Mol and Birkinshaw, 2008).

Typically, these innovations were incubated in organizations—like Midvale Steel, Ford, General Electric, DuPont, General Motors, Toyota and Amazon—and collectively they vastly multiplied human initiative, ingenuity and impact.

Management is humankind's most important technology, and sets the outer boundaries on what we can accomplish as a species. As management researchers, we have the great privilege, if we're willing to claim it, of helping to push those boundaries ever-outward.

Fair enough, you may be thinking, but you might argue that management is a mature technology; that there's little room left for fundamental innovation. We'd agree with that assessment, if by "management" you mean the top-down, highly stratified, rule-choked structures and processes that still predominate in most organizations around the world. But while we may have reached the limits of "bureaucratic managerialism" as a social technology, we don't believe for a moment that we reached "the end of management." Nevertheless, it will take bold thinking and doing to get management onto a new S-curve.

It's useful to remind ourselves that like all technologies, industrial bureaucracy is a product of its time. In the mid-19th century …

- Most employees were illiterate.
- Administrative skills were rare.
- Information was expensive to gather and move.
- Scale was a decisive advantage.
- Change was comparatively gradual.

None of this is true today. Nonetheless, our organizations are still stuck with a Victorian age management model. But we don't want to be stuck. That's why we flock to new management practices like moths to a flame—be it Spotify, Haier, or Buurtzorg, or in earlier years, WL Gore, Oticon and ABB. The danger is that as management researchers we end up as little more than journalists—aiding the process of diffusion but making little or no contribution to creating *new* practices (Birkinshaw, Hamel and Mol, 2008). Surely, we can do better than this—so let's go back to our starting question: What can management researchers do to achieve a ten-fold increase in the impact of their research? Herewith, a few suggestions.

Strategies for Multiplying Impact

#1: Aim Higher

The first step is to raise our sights.

We have long argued that no organization out-performs its aspirations (Hamel and Prahalad, 1989)—and the same is true for management researchers. Sadly, it seems that many researchers suffer from a kind of "ADD"—ambition deficit disorder. The operative, though seldom-stated assumption is that today's bleeding edge practices, whatever they may be, set the outer limits on what is possible. Few management scholars, in our experience, can see beyond "what is" to imagine "what could be." As long as this is true, we'll be followers.

We know of researchers in other fields who are working to clean up the world's oceans, create machines that can emote, master the intricacies of nano-scale manufacturing, and reverse climate change. The question is whether we, as management researchers, are working on problems that are similarly noble and inspiring?

As human beings, we measure ourselves not just by our accomplishments, but by the audacity of the challenges we take on. An out-sized challenged forces you to think differently, keeps you intellectually alive, and pushes you out of your comfort zone.

Each of us needs to ask, if I could choose to work on one truly momentous and worthy problem, what would it be? What challenge is worth the scarce currency of my life?

So another question: When you dream about the future of management, what do you dream about? How would you finish this sentence? “I dream about organizations that …”

For our part, we dream of organizations that …

- Change as fast as the world around them
- Are alive with the spirit of entrepreneurship
- Harness the genius of every employee
- Transcend seemingly intractable trade-offs
- Infuse work with dignity, opportunity and equity

Rather than starting with the status quo, we need to start with the dreamspace. That’s why, a dozen years ago we invited a group of amazing thought leaders to come together to define a set of challenges for management innovation. Participants included Chris Argyris, Peter Senge, Shoshana Zuboff, Henry Mintzberg, Yves Doz, CK Prahalad, Linda Hill, Andrew McAfee, Jeff Pfeffer and many more.

Together we identified 25 “moonshot” problems to solve—such as humanizing the language of business, developing internal markets and reinventing the means of control. You can find descriptions in an article Gary wrote for *Harvard Business Review* (Hamel, 2009). Your interests may point you in other directions, but know this: Life is too short to work on inconsequential problems, and your influence and capabilities are too valuable to be wasted on anything less.

As researchers, our first loyalty can’t be to a particular discipline, methodology or conceptual construct—but needs instead to revolve around big, chunky problems. We heartily agree with Don Hambrick’s (2005: 124) assertion that “[The most useful theories] don’t come from scholars struggling to find holes in the literature.” Instead, he argues that researchers should “start with a real-life interesting puzzle; then develop a preliminary set of ideas for solving the puzzle; and then turn to the literature for insight and guidance.”

Innovation is born in the gap between aspirations and reality—the bigger the challenge, the bigger the chance of a genuine breakthrough.

Let us share a story about an Indian physician, Dr. Govindappa Venkataswamy, the founder of the Aravind Eye Hospitals in India.

Back in the 1970s, Dr. V. had an ambitious goal: curing unnecessary blindness in India. Without protection from the sun, cataracts are likely to develop in older age—and as a result, millions of Indians suffered from blindness. To change this, Dr. V knew he had to dramatically reduce the cost of cataract surgery.

His breakthrough was to build a surgical model based on McDonald's high throughput service model. At the Aravind Eye Hospitals, which Dr. V. founded, patients are lined up on parallel beds. Equipment moves from bed to bed. Surgeons perform as many as 100 surgeries during each 12-hour shift. Operating rooms are in use 24-hours a day. This model has reduced the cost per surgery to around $35, 2% of the cost for a similar procedure in the United States—with the same medical outcomes. Each year, Aravind's five hospitals perform more than 300,000 surgeries.

Like Dr. V., we need to aim higher.

As Sir Peter Medawar, the Noble prize-winning zoologist once said, "Dull or piffling problems yield dull or piffling answers." So, if we want a ten-fold increase in the impact of our research, we need to be focus on achieving 10X gains in organizational effectiveness.

#2: Challenge Orthodoxy

After raising our sights, we need to challenge old paradigms, since it is our paradigmatic beliefs, more than anything else, that blind us to new possibilities.

Over time, a field of knowledge develops a high degree of internal consistency. That's a good thing. It allows knowledge to compound—but at some point, that internal consistency becomes a barrier to new thinking.

We can represent the structure of organizational knowledge with a simple framework.

Paradigm
↓
Problem
↓
Principles
↓
Processes
↓
Practices
↓
Performance

At the top is one's worldview, the basic assumptions one makes about the role of organizations in society, the most productive ways to organize economic activity, and the relationship between people and their organizations

These assumptions point you towards certain problems and away from others. Let's take an analogy …

If you had lived in the 17th century, and like John Locke, believed in the sovereignty of the individual—rather than of a monarch, party or state—you might have been drawn to the problem of constructing a system of self-government. That quest would have led you in turn to certain principles: like consent of the governed, one person one vote, universal suffrage, an independent judiciary, freedom of religion, and so on. You would then have sought to turn those principles into electoral, legislative and judicial processes which, in turn, would have spawned specific practices—like judicial appeal. Taken together, all of this highly influences the performance of a polity.

So it is with management. The problem for industrial bureaucracy was efficiency at scale—or, more particularly, how to minimize variances and maximize conformance. This problem led researchers and practitioners to embrace principles like stratification, standardization, routinization and specialization.

Over the decades, these principles have been deeply operationalized in processes—goal setting, resource allocation, performance management, and so on; and those processes, in turn have defined everyday practices—how a manager conducts a performance review, develops a budget request, or structures a team—and they, in turn determine performance.

As a knowledge system matures—as industrial bureaucracy has over the past hundred-plus years—performance gains get harder and harder to come by. As we saw, the disciplines of bureaucracy have produced stunning advances in labor and capital efficiency. But the rich seam of operational inefficiencies addressable by bureaucracy is mostly tapped out.

Over time, a system's performance becomes limited less by processes and practices than by paradigms and principles. Thus to get management onto a new S-curve, then, we must start by challenging our paradigmatic beliefs—which are often invisible to us.

For example, one of the canonical beliefs in the bureaucratic model is that people work for organizations, rather than the other way around. In this view, employees are "resources," or "human capital." They are instruments, employed by the organization to produce products, services and, ultimately, profit. Given this instrumentalist view of human beings at work, it's hardly surprising that, as per Gallup, only 15% of employees around the world are fully engaged—emotionally and intellectually—in their work (Royal, 2019). When people are treated like instruments, they seldom give their best.

Viewing people as resources may simply be a habit of mind, but we've met few researchers or business leaders who've explicitly challenged this assumption. An exception is Zhang Ruimin, the chairman and CEO of Haier, the world's largest appliance maker.

Sitting in Gary's California office a decade ago, Zhang said: “At Haier, we encourage employees to become entrepreneurs, because people are not a means to an end but an end in themselves. Our goal is to let everyone become their own CEO.” We don’t know many CEOs who channel Immanuel Kant and the categorical imperative, but if you want to reinvent management, it’s not a bad place to start.

When you shift the paradigm, new problems come into focus—like turning employees into entrepreneurs, and maximizing contribution rather than compliance; and those new problems, in turn, point you towards new principles—like ownership, meritocracy, markets, community, openness, and so on.

An aphorism often attributed to Thomas Kuhn holds that, “All significant breakthroughs are break-‘withs’ old ways of thinking.” If that’s true, we need to ask: Are we bringing fresh and radical thinking to management? Are we framing problems in new ways? Are we tacking novel challenges? And have those challenges sent us off in search of better principles?

One useful way to escape the straitjacket of existing paradigms and principles is to seek inspiration from other fields, such as evolutionary biology, urban development, theology, neuroscience, computer science, and so on.

For example, scientists long thought the structure of the brain was hierarchical, but this has proved not to be the case. The brain is a neural network, and the actual locus of control is constantly shifting. Could this sort of lattice-like structure be the model for tomorrow’s organizations? Does formal hierarchy need to disappear? We don’t know, but we suspect that the only way to get non-linear gains in organizational performance is to radically challenge traditional organizational models—and a good starting point is to study examples of “organizing” that are far afield from the traditional precincts of management research.

Indeed, given our “stuck-ness,” and the hegemony of the bureaucratic model, we need to ask: What do we really “know” about organizations, anyway?

While we can learn much from the giants of organizational science—individuals like Herbert Simon, Jim March, Peter Drucker, Henry Mintzberg and Oliver Williamson—it’s worth noting that virtually all of this knowledge was extracted from organizations that fit the same bureaucratic mold:

- Decision rights correlate with rank.
- Strategy gets set at the top
- Big leaders appoint little leaders
- Managers assign tasks and assess performance
- Powerful staff groups set policy and enforce compliance
- There are multiple layers of administrative oversight
- Employees are slotted into narrow roles.

Given the longevity and familiarity of bureaucracy, it's easy, though mistaken, to regard it as a cosmological constant.

More than a dozen years ago, Gary had a long discussion with Jim March, one of the founders of the behavioral view of organizations that remains dominant today. "Did he believe," Gary asked, "that we could change the fundamental nature of large organizations? After several minutes pondering the question, Jim said, "no." In his view, the strengths and weaknesses of our organizations are rooted in the frailties and proclivities of human beings.

But we wonder if this is entirely true. Yes, there are certain things that are immutable about human nature. Having said that, all of us are different sorts of people in different settings--Depending on the context, different attitudes, behaviors and even skills come to the fore. As Emerson Brown famously said: First we shape our tools, then they shape us. We've all been shaped by bureaucracy, but that doesn't make it inevitable.

Bureaucracy—with its formal power structure, cascading targets, elaborate rules and zero-sum promotion battles—rewards some behaviors and penalizes others. People who excel in a bureaucracy are good at managing up, negotiating targets, defending turf, deflecting blame, and managing the optics of their own performance.

But what Jim couldn't have seen at the time is how individuals might behave in a large organization like Haier or Buurtzorg, the Dutch home health provider, that are comprised of thousands of small, entrepreneurial units[3], each with its own P&L. In these organizations the work of managing has been almost entirely distributed to frontline teams. Leaders are chosen by the led and interdependencies are managing through peer-to-peer collaboration rather than via head-office mandates or additional management layers. Notably, both these organizations, like many other post-bureaucratic pioneers, enjoy substantial performance advantages over their competitors.

There's no way of getting management onto a new S-curve without abandoning the bureaucratic paradigm. Management researchers, as much as practitioners, are still stuck in Max Weber's "iron cage"—and the only way to build fundamentally more capable organizations is to stage a jailbreak.

Given that, the most important skill any young scholar can learn is to think like a contrarian.

#3: Invent

Having escaped the status quo, the next step is to invent the future.

A good idea without an instantiation isn't worth much. That's why the development team that birthed the first iPhone, in a bid to win support for their pioneering work, built a clumsy table-sized mock-up of a touchscreen.

Radical thinking is of little value if it doesn't lead to radical doing. And in the real world, as opposed to the ivory tower, having impact means building things. That's how you close the gap between theory and practice. So if researchers want to get the attention of practitioners, they need to show them something tangible—or better yet, co-create it with them.

Obviously, an invention doesn't have to be a physical thing. It can be a tool like Net Promoter Score, Social Network Analysis, or the Likert Scale.

Given the importance of building things, it's useful to make a distinction between two sorts of research: research that accounts for the way the world works and research that changes the way the world works. The former is a necessary precursor to the latter, but we believe management research has over-indexed on explanation.

During one of the Apollo missions, NASA set up a comms link between an astronaut and his young son on earth. "Daddy, the boy asked "who's flying the spaceship right now?" "At the moment," his father replied, "mostly Sir Isaac Newton."

The space program would have been impossible without a reliable theory of planetary motion. But it would also have been impossible without the inventions of Werner von Braun and his compatriots. Von Braun, who came to the US after the Second World War, was the chief architect of the Saturn V rocket that took the Apollo astronauts to the moon. For his contributions, he won the US National Medal of Science.

Management science is applied science—not solely, but mostly—or at least it should be. So when asked, "Are you a theorist or a builder?," the right answer is "yes."

In the process of knowledge creation, discovery and development are deeply intertwined. We love the mantra of our friends at IDEO, the design firm: "Build to Think".

The best way to test your theory is to try to turn it into something useful. Often, in doing so, you are forced to amend or even abandon your initial theory. It's no accident that many of the most important theoretical breakthroughs in science came from experiments gone wrong— from anomalous results that forced a theory re-think.

Years ago, Gary had the chance to test his ideas about open strategy when he led an experimental project to help Nokia build its first mobile phone strategy. This experience was the backdrop for a later article, "Strategy as Revolution" (1996)

He also had the chance to co-develop one of the world's first innovation markets, for Shell—christened Gamechanger—which is still going strong. That experiment was the impetus for an article on internal markets: "Bringing Silicon Valley Inside" (1999).

Back In 2008, we co-founded the Management Innovation Lab at the London Business School, launching a range of innovative management experiments In leading companies such as Roche and UBS. More recently, Gary and his colleagues built a collaborative platform, CrowdLab, to test new approaches to large-scale problem solving. In one setting, 3,000 employees used the platform to hack their company's management model (Hamel and Zanini, 2020).

We don't claim be particularly adept at inventing useful new management tools. The efforts described above have been sporadic and halting. Nevertheless, the best way to fully understand a system is to seek to change It. Extending the frontiers of knowledge involves both investigation and intervention, but as management researchers we have too often been content to be mere observers.

So how do you set about inventing new management techniques? A good starting point is to become deeply familiar with the tools and technologies that support what is popularly known as the "social web." These include microblogging, P2P markets, wikis, user ratings, socialgraphs, virtual communities, opinion markets, *et al*.

While bureaucracy was undoubtedly the most important social technology of the 20th century, the social web, powered by the Internet, is the most important social technology of the 21st century—and has profound implications for how our organizations are built and run.

Organizational innovation often lags technological innovation by decades, (see, for example, Knox and Murray, 2001), and that seems currently to be the case. At the moment, organizations are using social technologies mostly to improve team productivity—to share documents, align calendars, set up virtual meetings, and track progress. That's fine—but as of yet, few companies have used these tools to create strategy, allocate resources, make leadership appointments, apportion decision rights, or support large-scale change efforts.

Decades of organizational research has identified profound difficulties with performance assessment, leadership development, allocational efficiency, strategic resilience, and more. We believe that technology can help us overcome many of these problems.

#4: Experiment

At this point you may be thinking that applied research—tool building—is beyond the scope of a business school, we would like to disagree. While a relative novelty within management faculties, applied research is often vibrantly embraced by other disciplines.

For example, Stanford University has more than a dozen multi-disciplinary research institutes, including:

- Center for Biodesign
- Genome Technology Center
- Institute for Human-Centered AI

- Nanoscale Prototyping Library
- Center for Molecular and Genetic Medicine
- Center for Mind, Brain, Computation and Technology

Read the mission statements for these centers and you see a commitment to …

- “Developing tools”
- “Designing solutions”
- “Creating instruments”
- “Pursuing breakthroughs”
- “Identifying technologies”
- “Creating prototypes”
- “Shaping the future”

How often are words like these used to describe the goals of management research? Not often enough, we reckon.

Stanford’s research centers are testament to a simple truth: Experimentation is essential to the advancement of knowledge in any field of applied science.

There has of course been a boom in experimental methods across the social sciences over the last fifteen years or so. But we need to be clear on what we mean by an experiment. Broadly speaking, there are three types.

- Lab experiments, where subjects (usually undergraduate students) perform tasks under controlled conditions, with treatment and control groups and random assignment.
- Natural or quasi-experiments, where there Is an external shock that acts as a treatment effect on one sub-group of people or firms, while another sub-group Is unaffected.
- Field experiments, where the researcher works within a business or governmental organization to designs a treatment for a randomly-assigned sub-group, to test a specific hypothesis.

Each of these has Its pros and cons. Lab experiments hit the 'gold standard' of randomized assignment but they focus on very narrow interventions under somewhat unnatural conditions that may not translate into the real world. Natural experiments provide Insight Into messy real-world phenomena, but the researcher Is detached, observing from a distance, and limited to studying situations where suitable external shocks can be found.

We contend that interventional field experiments are the potentially the most Informative and impactful means for advancing theory and practice—because they represent a *bone fide* attempt to change things for the better while still upholding the highest standards of academic rigor. A salutary example is the study by Nick Bloom and his colleagues, (*Does Working from Home Work?*, 2015), in which they randomly assigned half of a group of call center workers in a Chinese travel agency to work from home for nine months while leaving the control group In the office. It was a simple design with clear findings, and the company wasted no time in capitalizing on the experimental findings.

Unfortunately, field experiments are few and far between. In the leadership field, a paper by Philip and Nathan Podsakoff (2019) showed that between 2015 and 2018 natural or field experiments accounted for fewer than 2% of the papers published in leading management journals.

In the strategy field, Ronny Chatterji and colleagues (2016: 117) found a total of 30 field experiments published in top journals from 1984 to 2014[4]. As they noted, "the number of papers published based on field experiments or randomized controlled trials has exploded in economics journals, [but] no corresponding trend has occurred in strategy and management journals."

So why so few field experiments? In part it's a matter of access and credibility. It's not easy to persuade a company to run management experiments, even small ones. It's also true that the data extracted from a field experiment can be more ambiguous than that gathered from a well-controlled lab study. But it's not clear that these difficulties are any more daunting than those faced by researchers in other fields. In their quest to understand the elementary particles of the universe, curious physicists somehow found a way to fund, design and build the world's biggest machine—the Large Hadron Collider. Given the importance of organizational innovation to human progress, there's no reason management researchers should be any less ambitious.

The pace at which management evolves is ultimately gated by the number of smart, paradigm-busting experiments that are run each year. Obviously, in their quest to improve performance, organizations run thousands of informal experiments each year—though most are not recognized as such, are not particularly novel, are not carefully constructed, and are not mined for generalizable findings. It seems to us that those trained in the art and science of organizational research could do much to increase the number, pace, value and usability of the "native" experiments that organizations launch each year.

Sadly, the dearth of field experiments in leading journals means that Ph.D. students and young faculty are likely to face difficulties in finding mentors who can guide them in building and executing interventionist studies. Looking at the literature, up-and-coming faculty are also likely to conclude that field experiments are somehow disadvantaged in producing robust findings and are a perilous route to publication.

This creates a self-reinforcing cycle—few field experiments, few publications, few mentors, doubts about efficacy, and so on. As long as this cycle persists, the impact of management researchers will be modest at best, and negligible at worst.

#5: Collaborate

Collaboration is a fifth strategy for multiplying impact.

In recent years “big science”—large, collaborative efforts like the Human Genome Project—have proven highly effective in addressing complex problems—like unpacking the human genome.

The Atlas Project at CERN, which confirmed the existence of the Higgs bosun, brought together more than thousands of scientists from 183 institutions. To coordinate this vast effort, the researchers organized themselves into a community of communities, with each subgroup responsible for developing a particular subsystem of the Atlas detector and running particular sorts of experiments. The paper which announced the Higgs discovery had more than 3,000 co-authors.

Another grand collaborative effort is the Human Brain Project, which encompasses 500 researchers from over 100 universities. The goal is to build the datasets, tools, models and infrastructure to support breakthroughs in brain research.

Such efforts are exceedingly rare in the field of management[5], but we think it’s worth asking whether a similarly large-scale effort in organizational science would help to accelerate the development of post-bureaucratic management tools, methods and approaches?

Even apart from “big science,” management researchers need more leverage. Gary has a young relative who’s working on a PhD in theoretical chemistry at one of the world’s top research universities. Her Principle Investigator is supervising more than 30 PhD students, postdocs and lab assistants. This extended team gives the PI extraordinary research leverage—and yields a torrent of knowledge and papers. Labs such as this are typical in the hard sciences, but seldom found in management research[6]. In the typical business school, experienced researchers of great imagination have only slightly more research leverage than junior faculty.

In consequence, enterprising researchers sometimes build their own mini-research centers outside the university setting. Academic purists might view these entities as consulting ventures, but in our experience they served first and foremost as laboratories. They provided a legal vehicle for engaging with sponsors, hiring experienced researchers, and contracting with software developers and other vendors. We have long been frustrated that all this can’t be more easily accomplished within a business school setting. One research dean told us that that any interventional study supported by corporate funds was, by definition, consulting rather than research. We’d like to believe such attitudes are rare, but we suspect they’re not.

Theory *and* Practice

Aspiration, heterodoxy, invention, experimentation, collaboration—this is the recipe for radically multiplying the impact of management research. But one also needs willing cooks—researchers who are eager to engage in the world of practice. Unfortunately, this willingness often seems lacking.

Some management scholars, it seems, believe practical knowledge exists “downstream” of theoretical knowledge—rather than in a mutually-reinforcing, generative loop. This is the “trickle down” model of management research, and if you think it’s a caricature, read Daft and Lewin’s article (2008) on “Rigor and Relevance in Organizational Studies.”

The authors argue that when it comes to management research there are “two sorts of relevance.” An article can have “(1) relevance to the research of other academics, or (2) relevance to the problems and practices of people managing organizations.” Note the use of the word “or.” The assertion is that, with perhaps the odd exception, an article that’s relevant to academicians won’t be relevant to practitioners, and vice versa.

Not surprisingly, Daft and Lewin, believe that academic relevance is “a sufficient criterion for publishing an article in an academic journal.” But they go further than that when they assert that, “[It is] unrealistic … to aspire to simultaneous academic and managerial relevance.”

This is stated so categorically, that it’s clear the authors see the problem of incompatibility between theoretical and practical perspectives as more than a matter of style—it entails more than the need to adjust one’s prose to a particular audience.

Some might argue that this supposed incompatibility stems from the scientist’s commitment to rigor, but in our experience, few practitioners are clamoring for sloppy research. Quite the contrary. In executive education and other settings that frequently push academics to back up their assertions with hard evidence. Nor are the phenomena that occupy organizational theorists so irreducibly complex that, like quantum effects, that they can be fathomed only by savants.

Nowhere in their article do Daft and Lewin explain *why* they believe academic and managerial relevance are constitutionally incompatible; nor, if they’re right, why researchers should be untroubled by this fact. That omission leaves us free to speculate.

Perhaps—and this is just a hypothesis— there are some in the academic community who’d rather not do the hard work of immersing themselves in the world of practice; who don’t want to be burdened by the need to be relevant; who yearn for the luxury afforded to theoretical physicists when they debate the merits of string theory and the multiverse; or who simply get an ego boost by exaggerating the distinction between the ecclesiastics and the laity.

For any or all of these reasons, a long-tenured faculty member might want to propagate the notion of an unbridgeable chasm between thinkers and the doers. And of course, with enough effort, it is, indeed, possible to build an intellectual fortress that is so far removed from the paths of ordinary folk, or so forboding in its construction, that few venture near.

With apologies to Stephen Jay Gould (1997), and the distinction he draws between science and religion, management research and management practice do not inhabit “non-overlapping magisteria.” And it seems deeply unhelpful (if not self-indulgent) to assume otherwise.

Like bureaucrats who've mastered the art of political infighting, senior researchers, who've mastered the rules of writing for other, similarly detached theorists, aren't eager for the game to change. While bureaucrats cloak their self-interest with the claim that bureaucracy is unavoidable, the cardinals of academia fall back on the claim that their commitment to "pure science" absolves them of any responsibility for relevance. That's a bogus claim and needs to be confronted as such.

Ultimately, management researchers are accountable not to colleagues, deans, editors and reviewers, but to all those around the world who are working in organizations that aren't as capable, humane and productive as they could be.

Conclusions

We have said little about how to operationalize all this—not because we haven't thought about it, but because our goal here is to provoke not prescribe. Clearly there are some obvious opportunities for individual researchers to commit themselves to tackling bolder research questions, and for business schools to borrow lessons from experimental research endeavors in other fields. It's also heartening that here and there one sees initiatives underway that are pushing in the right direction[7]– but much more needs to be done.

We dare to hope that there are other researchers out there who are similarly eager to help invent the future of management and simiarly frustrated by the present rate progress, who want to get management onto a new S-curve, and who share a belief that it's time—past time—to challenge the status quo.

Perhaps together, we can set the trajectory of management on a new course—and build at last, organizations that are fit for the future and fit for human beings. That's our dream; we hope it's yours.

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Endnotes

[1] NSF funding reports can be found on: https://www.nsf.gov/about/history/annual-reports.jsp
[2] There are differing views on this point. For example see: Khurana, Rakesh, and Nitin Nohria. "It's time to make management a true profession." *Harvard business review* 86.10 (2008): 70-77; Barker, Richard. "No, management is not a profession." *Harvard business review* 88.7-8 (2010): 52-60.
[3] The case studies of Haier and Buurtzorg are described in Humanocracy (Hamel and Zanini, 2020)
[4] Since 2014 there has been a slight uptick in the cases of field experiments finding their way into strategy journals, but still very small numbers: for example we found 6 field experiment in Strategic Management Journal from 2014 to 2020, and 10 in Academy of Management Journal in that same period.
[5] For example The Globe Project (www.globalproject.com) and the Global Entrepreneurship Monitor (www.gemconsortium.org)
[6] For an interesting argument for why we see different models in the physical and social sciences see: Rahmandad, Hazhir, and Keyvan Vakili. "Explaining heterogeneity in the organization of scientific work." *Organization Science* 30.6 (2019): 1125-1145.
[7] A few examples we are aware of: The Laboratory for Innovation Science at Harvard (www.lish.harvard.edu), The MIT center for collective intelligence (www.cci.mit/edu), the open innovation forum at Cambridge University, https://www.ifm.eng.cam.ac.uk/research/ctm/openinnovation/